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### *Pomo Lineages: 'Why Not?'* *A Response to Kunkel*

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I found Kunkel's article, "The Pomo Kin Group and the Political Unit in Aboriginal California," to be most interesting and stimulating (see Vol. 1, No. 1 of the *Journal*). I do trust that, even though only an interloping Africanist of sorts, I may be allowed to offer some criticism of certain of his conclusions. Insofar as I am qualified to say, I found his ethnographic case for the existence of "ambilocal residential kingroups as . . . basic [Pomo] political subdivisions" quite convincing. My reservations primarily concern his contention that the finding is inconsistent with the existence and/or political significance of unilineal kin groups.

As an Africanist (even if only "of sorts"), my intuitive reference point is Evans-Pritchard's *The Nuer* (Oxford University Press, 1940). The Nuer are supposedly the "type case" of a patrilineal society in which the lineage system provides both the political units and the structure according to which relations between units take place. The Nuer lineage system is explicitly presented by Evans-Pritchard as mapping from a genealogy onto the actual ground, and thereby defining socio-political-spatial units. Consistent with this social model, the Nuer are described as having a patrilocal residence rule.

It is relevant, thus, to look at what Evans-

Pritchard tells us about actual residential patterns. A man and his wife *should* live with his father and brothers. But either because of ecological/economic problems or because of quarrels with these kinsmen, a man often has to live elsewhere (e.g., with his matrilin or with his affinal kin). Evans-Pritchard indicates that such residential decisions have happened often enough that only a relatively small minority of the residents of any one locale are likely to belong to the lineage with which that locale is associated.

In wars or fights, the Nuer's major "political" activity (as described by Evans-Pritchard), the sides are spoken of as 'lineages,' but it seems to be clear from Evans-Pritchard's account that it is in fact spatial or residential units representing the lineages that actually battle, especially at the levels—above the level of fights within the household or immediate hamlet—that actually seem to represent political organization.

The Nuer lineage actually associated with a local political unit "include[s] only certain families, excluding others," as does the "functional family" which Kunkel quotes McKern as describing for the Pomo-like (?) Patwin. McKern (and presumably Kunkel) consider that the only partial overlap of "functional family" and "residential kingroup" prevents the "functional family" from being "the social unit of the community structure," yet the similar Nuer lineage seems to represent precisely such a unit. McKern's other reason for not considering the Patwin "functional family" to represent a "politically significant lineage" was that "although" its special at-

tributes "added to the social prestige of a family, it yielded no political powers or influence as such"—again exactly paralleling the Nuer situation.

In sum, the Nuer "basic political subdivisions" are "ambilocal residential kingroups" in exactly the same sense as are Kunkel's Pomo subdivisions. But the Nuer are normally considered to be strongly unilineal, and to have a political structure based directly on their patrilineages. It is of further interest to note that these Nuer lineages have exactly the attributes which cause McKern, with Kunkel's approval, to assert that the Patwin "functional family" is not "a politically significant lineage."

There are at least two reactions that one could have to the above parallels. One could simply decide that Evans-Pritchard was wrong about the patrilineality, patrilocality, or lineage political significance of the Nuer. By Kunkel's (and McKern's) criteria such would be a reasonable conclusion. But since Evans-Pritchard's Nuer work, along with Fortes' Tallensi work, has played a major role in delimiting these concepts for anthropology, such a solution would have the effect of depriving the notions of "politically significant lineages" and "uni-locally based residential groupings" of any empirical content.

The other reaction would be to decide that Kunkel's (and McKern's) definitions are wrong, and thus that their excellent ethnographic findings do not necessitate the inferences which they would wish to draw. While I do endorse this latter reaction, I also want to show that it does not reflect any lack of appreciation of Kunkel's interesting and stimulating article. The stimulation comes because the definitions used by Kunkel and by McKern are *not* the products of any egregious misreading of anthropological theory or of the specific theory developed and espoused by Evans-Pritchard and Fortes. It seems to me that the kinds of definitions used by Kunkel

and McKern represent the kinds that Evans-Pritchard and Fortes felt *ought* to occur, and so represent the kinds which structured their theoretical work to some large degree. The question raised, thereby, is "if not the definitions used by Kunkel and by McKern, then what definitions"? Evans-Pritchard, in his Nuer book, does point in the direction of an answer: he writes as if Nuer patrilineality (at the political level) and Nuer patrilocality were not so much what the Nuer actually do as an interpretive framework by which Nuer remember their obligations and explain their actions. Fortes, on the other hand, seems to have preferred a "literal" world, more like that expressed by Kunkel and McKern.

Kunkel's article, in the context of Evans-Pritchard's Nuer work, makes clear the definitional problems that inhere in discussions of unilineality and unilocality (even as in much of the rest of anthropological discussion). One aspect of the problem has been pretty clearly explicated in the course of the famous Goodenough/Fischer controversy about the classification of residence types. Such classifications can either be based on a consideration of who is actually living with whom or on the decision rules or processes by which residences are chosen. These two kinds of criteria do *not* produce equivalent classifications since the kinsmen on whom a residential decision is based may not actually be residents of the household in question (*via* other obligations or death) or may not be the (apparent) household heads. In addition, even people who have unilocal residential rules also have contingency rules in case the major rule cannot be followed for one reason or another. The Nuer practice of going to live with maternal or affinal kin upon getting into a serious fight with paternal kin apparently represents such a contingency rule. Thus, even the careful census data on the actual constitution of Pomo residential groups collected by Gifford and cited by Kunkel is insufficient to estab-

lish what the Pomo "residential rules" were. One avunculocal pattern (which I have observed among the Fanti of West Africa) has sons (in a matrilineal society) living with their father until his death, and then going to live in their maternal uncle's home—even if he, too, has died in the meantime. The Gifford data, as cited by Kunkel, seem not to be inconsistent with such a pattern, since the members of a "hearth group" are predominately paternally related to their head, while the different "hearth groups" that make up a house are predominately maternally related to one another.

In general, the question boils down to the old one of whether we wish to describe ethnographic facts in the categories used by the natives or in categories derived from some external theory. In general, there is no single best answer; we describe events one way, the other, or even both ways according to our specific problem.

We are now in a position to see why I prefer Evans-Pritchard's definitions to Kunkel's. We know that the Pomo have residential groups which represent the basic units of their political structure. The question is which definition of these groups is more useful and informative. In the Pomo case, a classification, on Kunkel's criteria, of "ambilocal" tells us nothing. We would like for a characterization of residence patterns (in a context in which they produce the basic political units) to tell us who will be living in a given residence, when, and on what basis. Kunkel's simple summary description of who is living there does not give us this information. On the other hand, a decision rule (implicit in Evans-Pritchard's description of Nuer norms and actions) does give us a basis for understanding and predicting individual residences. Native actors' decision criteria are not automatically best for all purposes; psychological theories of the affective effects of coresidence obviously depend on knowledge of who is

actually living with whom. Similarly, we need external information to predict when and for what reason a society will change its residential norms—even if native actors' concrete implementation of the norms depends on their internal decision procedures.

In this view, the basic classification of a society's residence pattern should represent the ideal decision or the pattern that would result if everyone were able to follow the norm—i.e., the basic decision rules without the contingency rules.

Kunkel is making two separate claims about the Pomo. He is claiming (1) that they have no unilineages, and (2) that, *even if they have* such lineages, the lineages cannot provide the basis for Pomo political organization because (a) Pomo political organization is known to be based on residential groups, and (b) these residential groups are not based on any pattern of unilocal residence as they would be if the local groups were lineages. He has no evidence for the first claim, which forces him to fall back on the second, weaker, claim. His evidence for the second claim rests on the mixture of residence modes exhibited by Pomo residential groups. But, as we have seen, in order to answer the question of whether or not lineages formed the basis of these residential groups we need to know (a) whether the Pomo thought they had lineages, (b) whether they had a normative association of a lineage with a residential unit, and (c) what their residence rules are (both *ideal*, to define the pattern, and *contingency*, to account for actual residences) since these rules *could* account for the non-unilineal constitution that the Pomo groups actually exhibited on the ground. But this information about Pomo norms and rules is exactly the information which is lacking. Thus, it seems to me that it is *impossible to rule out* any unilineal basis for Pomo political organization on the basis of Kunkel's data—we simply cannot tell what they had. In terms of wider questions,

the Pomo might actually have been in a social/ecological situation that really called for a mixed residential/political strategy. But, on the other hand, they also might have been in a situation that called for a clear unilineal strategy, but wherein individual actors or groups had particular problems (e.g., quarrels or micro-demographic variation) which interfered with their execution of that strategy.

It is this missing information, in the context of California's ecology, which would allow California Indian studies to make the kind of important substantive contributions to social theory that Kunkel and I would both like to see. Kunkel's article, by highlighting the theoretical issues and by clarifying the ethnographic facts, has materially increased the likelihood of such a contribution.

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## ***Reply to Kronenfeld***

PETER H. KUNKEL

Kronenfeld raises the question of whether the existence of ambilocal residential kin-groups among the Pomo is necessarily inconsistent with the existence of unilineal kin-groups. He bases his argument for the possibility of Pomo lineages on analogies between Pomo and Nuer residence patterns. Implicit, however, is a hidden assumption, namely that "primitive peoples" are not capable of establishing social order beyond the level of the "band," without recourse to the principle of

lineal descent. My paper on the Pomo kin-group, etc., was intended as an attack on Service's hypothesis of the universality of the patrilocal band among truly aboriginal food-collecting societies. Unfortunately, I did not also make explicit my opposition to the inevitability of unilineal descent at the "tribal level" (another hypothesis of Service, apparently shared by Kronenfeld).

A detailed rejoinder to Kronenfeld will probably serve as a convenient vehicle for demonstrating the weakness of the unilineal bias. This is particularly convenient since Kronenfeld chooses his stance as an "Africanist of sorts." Because of the prevalence of lineal descent systems in Africa, and because of the considerable respect we all have for the theoretical contributions of Evans-Pritchard and other British social anthropologists who have analyzed African political and kinship systems, it is not surprising that some American theorists have taken the African data as "an intuitive reference point" from which to look at "tribal," kin-based social systems elsewhere. Thus Service, in *Primitive Social Organization* (Random House, 1962), posits a universal evolutionary progression from societies with patrilocal bands to societies with patrilineal lineages. In this scheme, matrilineal systems are viewed as a possible logical derivative, somewhat later in the evolutionary sequence. Still later, according to Service, non-unilineal systems emerge as quite ineffective adaptations to early acculturation circumstances. Even then, Service argues, non-unilineal systems have underlying lineal organizations, or traces thereof, presumably representing their prior conditions.

The basic reason many theoreticians assume a kind of inevitability for unilineal descent is that such systems are relatively easy for members to "remember," as compared with "bilineal" systems. This is so because a non-unilineal system is not really bilinear at all; the number of potential lines through